PROCEEDINGS

OF

The American Microscopical Society

MINUTES OF THE EIGHTEENTH ANNUAL MEETING,

HELD AT

Cornell University, Ithaca, N. Y., August 21st, 22d and 23d, 1895.

WEDNESDAY, *August 21, 1895.*

The members assembled in McGraw Hall at 10 minutes before 10 o'clock; about 60 persons present.

The meeting opened with the following address of welcome by the Hon. D. F. Van Vleet, of Ithaca:

Mr. President, Ladies and Gentlemen of the Society:

When I was asked, a few days ago, to make a formal address of welcome to you, I suggested to one of my good friends that, while it was something not new to my line, it would be the first time that I had submitted my humble remarks to 75 or 100 microscopists. I assure you that I feel rather diffident about talking to you; and yet I have words of welcome for you, which are of the best which I can give you.

I realize the fact that there is no body of scientists in the world who are such close observers, who have done so much and are to do so much for humanity at large, as your own society. In looking back over the history of microscopic investigation, I am amazed, upon a hasty examination, to see the great improvements which have been made in the microscope. There is, I take it, as much difference between the magnificent instruments which you have to-day, and which you are using, and the old instruments of 20 or 30 years ago, as there is between the old-fashioned churn

of our grandmothers and the magnificent apparatus for separating cream and butter from milk which you may see in an adjacent building. There is nothing manufactured, to my mind, where such enormous strides have been made in development, as with the microscope. I looked over hastily last evening one of your earlier reports. I found there that a great deal of time was given to the discussion of the proper forms of the microscope and its mechanism. Then out of curiosity I turned to your last report, and I found there that you are discussing questions of the greatest importance to the human race; so that I take it that you have now, ladies and gentlemen, a perfect piece of mechanism; and your usefulness as a society, it seemed to me—as a layman intensely interested in the work which you are doing—your usefulness to humanity at large is but just beginning.

Already, if you wish to see an object lesson, let me state to you that this little city where you are welcomed as guests, is expending something like $500,000—just beginning the work. And why?—because you, ladies and gentlemen of the microscope, have shown to the people that there is that in sewage which is an enemy to the human race; and so all over this country, not only in the little city of Ithaca, but everywhere, the results of your investigations are bearing fruit for the bettering of mankind.

Therefore, while your members are increasing, while the work that you have done in the past has been of the greatest importance, I predict that the next 10 years of your existence as a society will bring about far greater results than ever before. I do not believe that there is a body of men in the United States who have it within their grasp to do more for humanity, to do more for the world at large, than your association.

Therefore I hope and trust that your deliberations will continue year after year, that you will continue to do the good work already started and which is as yet in its infancy.

Now, in behalf of the mayor of this, our beautiful city, it is my function to welcome you. Ithaca is one of the most beautiful, one of the proudest little cities on earth. It is proud of its hills

and its valleys, proud of its lake, its streams, its waterfalls, proud of its university, and proud to be the host of guests like you. In behalf of the mayor, therefore, of the city of Ithaca, I extend to you a most cordial welcome. We welcome every year here hundreds of guests; we were never so proud to welcome a body of people as we are to welcome you, the leading scientists of America, who have seen fit to honor us and the great university which we love, by your presence.

In behalf of the 3,000 alumni who have graduated from the halls of this university, and who love it as they love nothing else on earth except their families, in behalf of that body of alumni I welcome you. There have gone out from these halls men well equipped to grapple with all the problems of this life. Some of them are of your number; and the alumni of Cornell University, young though they are in years, stand before you, representative scientists, as men who have taken the front rank. Cornell University has graduated a Jordan, a Comstock, a Gage, your honored President; and I might stand here an hour and tell you, ladies and gentlemen, what the alumni of this university have done. Therefore, in behalf of them, and I am sure every single one of them would like to be present here and greet you and take you by the hand—in behalf of them, as their present executive officer, I greet you and welcome you, and say to you that we desire, when you leave this, our alma mater, that you will carry with you the idea that Cornell University is well equipped to do the great work that it is doing, and that it is doing much to further the interests represented by the society to which you belong.

Then, too, in behalf of the President of the university, who is absent and who has delegated to me the task of welcoming you; in behalf of the university I would state to you that the university is open to you, every department of this great university greets and welcomes you. I doubt—of course I am prejudiced, but I doubt if anywhere throughout the length and breadth of this great land there is such another institution of learning as this institution whose guests you are. Each of you should consider yourself a guest of this university. You are here once

out of 18 years—I think this is your 18th annual meeting. I am sure that I voice the sentiment of Cornell University when I say to you that it would be the greatest pride to the university if every meeting of your association might be here. I am sure I voice the thought of the President of the university and of its professors and of its alumni when I say that if this association could meet here every year, and make here every summer a great department of a great university, it would further the ends for which you are working, that it would be of greatest advantage to you, that it would be of greatest honor to Cornell University.

In behalf of the university, therefore, in behalf of every citizen of this little city, whether great or small, I desire to welcome you, to say to you that our latch-strings are all hanging out, that we desire among other things most of all to greet you, to meet you, to become friends, to know you, to feel that we are honored in doing so. Your time here is limited. We regret it. We wish that instead of three days your stay could be extended to three weeks. We wish that all the beauty of this most beautiful country, that all the advantages of this, one of the greatest universities, all of its departments, everything which it has stored here in its laboratories and in its museums, might be yours to inspect and enjoy for weeks, aye, months.

Again expressing to you for every citizen, for every alumnus, for every person connected with Cornell University, a most hearty welcome, I venture to say that it is our belief that when you leave us to go away you will come back to us again, and you can not come back too often, you can not stay too long.

The President, Professor S. H. Gage, then made the following response to the address of welcome.

Mr. Van Vleet:

In behalf of this society I want to thank you most cordially for this welcome. Last year, owing to circumstances over which we had no control, one of the accidents that it is impossible to foresee, our meeting was not as successful as in some years. There was a feeling on the part of the American Association for the Advancement of Science, that great association in this country which

covers all knowledge, that it was time that it carried its benefits out on the Pacific Slope. You know, sir, that we have sent some of our good men out there to carry the good tidings; they have been welcomed, and the whole association was going there. This society was going as a feeble part of it. Owing to hard times and various other things, that desire was not consummated, and the American Association is now to meet in Springfield. The question then arose, where shall this society meet? It seemed to me, sir, that as so many members of the society whom I had met pleasantly had asked about the university, that they would like to come here to it; and I wanted them to come here to see what our laboratories are like, to see our beautiful campus and city. I went to the president of the university and asked him concerning it, and I was surprised at the heartiness with which he acceded to every suggestion and the earnestness with which he said that of all things which could come to the university such meetings would give him the greatest pleasure. I went then from him with his assurance as head of the university, to the various professors, and everywhere was the same cordiality, the same readiness to do everything that was possible to help in making the meeting a success and in greeting its members. Then some of us went down town; and with some hesitation, perhaps you remember, I went to you, and asked you to be a member of the local committee.

At your hearty consent to help us I felt encouraged. The chairman of the local committee, Dr. Rowlee, of whom I can not speak too highly for the efforts he has made, went to other men in the town. The same cordiality was met everywhere; and so instead of there being town and gown in this welcoming of us, there is only one, either all town or all gown, whichever you wish to call it.

Now, then, to come back to the serious question, that is, whether this society, whether we are worthy of this confidence, this cordiality. In the past, as you have said in your remarks, there has been work done in the society which I think has gone for the advancement of knowledge. The question is, the main

question for us as a society is, whether in the work which we are still to do are we going to add to human knowledge? If it does add to human knowledge, and therefore to the possibility of human happiness, it will add also to the security and honor of the nation. I feel sure, sir, that I voice the sentiment of every member of this society in saying that we will do everything we can to be worthy of your confidence, of your cordiality. And thanking you again for this greeting to our society, I now declare the meeting open for the business of the day.

The names of a number of new members were then read as recommended by the executive committee, and by resolution the secretary cast the ballot of the society for them and they were declared elected. They will be found in the list of members at the end of the volume.

Mr. Pflaum, of Pittsburg, then read a paper on "Alleged Meteoric Dust."

After the reading Prof. Kellicott said: I would like to inquire the relative size of the particles found in Calcutta and at Pittsburg.

Mr. Pflaum: The size given for those there was from $\frac{1}{100}$ of an inch to $\frac{1}{250}$ of an inch. But I found them far smaller. The very smallest were but $\frac{1}{600}$ of an inch. I do not believe that the slide I have here gives the smallest, because I tried to seperate as closely as possible the dross and dust from the shot itself. Of course there were larger ones too. But the size seems immaterial. Under what conditions these grains are found I do not know. But I imagine this: The iron and the silicious matter in the iron becomes liquefied by the heat. When it rises from the chimney it is cooled a little and under pressure from all directions takes on a spherical form.

Mr. Seaman: There is only one question regarding this paper, which seems to be of peculiar interest, and that is the way these particles may be transported, not necessarily from the Pittsburg blast furnaces, but even from those somewhere nearer by. Now some of you may remember a few years ago there was an eruption in the Indian Ocean that is known by the name of the eruption of Krakatoa. I think very few in this country have any

idea of the tremendous character of that eruption, which threw such volumes of dust into the atmosphere that it was supposed fully one half of the surface of the earth had the sun somewhat obscured and a yellowish haze in the atmosphere from the dust with which the air was filled by that eruption. I am not prepared to say that this was satisfactorily demonstrated, but it certainly is the best explanation which has been given of certain atmospheric phenomena observed at that time. Of course that eruption was upon an enormous scale—a scale that has not been paralleled since the destruction of Pompeii. Those of you who wish to look into that matter will find in the *Cosmopolitan* for April, 1895, a description of that scene by an eye-witness, which certainly surpasses anything I know in literature since the description of the destruction of Pompeii by Pliny. It will give you an idea of the immensity of that phenomenon by which the air was filled with this fine dust.

Mr. Hermann Schrenk then read a paper on the "Corky Outgrowth of Roots, and their Connection with Respiration."

Professor Gage said at the end of the paper: I imagine some of you who have been listening to Mr. Schrenk's paper have been somewhat astonished by his speaking about plants using oxygen. I was brought up on the pleasing theory that plants do not breathe oxygen, but carbonic acid gas, and only animals oxygen. Mr. Schrenk has been talking to you about organisms developed by plants for the purpose of breathing oxygen. If you think for a moment that the use of carbonic acid gas for a plant is simply like our use of beaf steak, if you remember that plants, just as we do, require oxygen to breathe, you will not be disturbed by thinking that Mr. Schrenk, who used to be a student with us here, has become unsound in his views by going to Harvard.

Professor Rowlee: I have been very much interested in the paper by Mr. Schrenk. It will be remembered by some members of the society that I presented a paper upon this general topic years ago, and at that time I looked up the literature relating to the subject somewhat carefully. I did not then, nor till Mr. Schrenk wrote me, anticipate that these structures would be

found in so many plants. There is a great deal yet, I think, to be learned about the life histories of our marsh plants—it is one of the most interesting fields for study.

The position that Mr. Schrenk takes with reference to these organs, these peculiar developments, may be strengthened, I think, by the general statement that these cells show great activity. They are cells that are highly charged with protoplasm—not devoid of protoplasm as are some cells of the stem structure. Now if this development were for floating purposes, as has been urged, for the purpose of keeping the stem at the top of the water, there would be certainly no occasion for unusual retention of protoplasm and activity in it.

I may add, however, by way of contribution to the subject that, being considerably interested in the matter of this development of tissue, I thought that it might possibly be developed by any plant, that any plant if submerged in a flood might develop it. We frequently have floods in Ithaca, and when we do there is an exellent opportunity to study the effects of submergence upon the lowlands here. I think that it was perhaps two years ago, after the vegetation was well started upon the alluvial bottom toward the lake that there came a very heavy rain so that the plants, many of them, were nearly submerged; I presume the water was nearly eight inches deep. I visited that region repeatedly with the special object of finding plants adapting themselves to that submergence. The upland plants had started to grow in normal terrestrial conditions, and they were submerged. To my surprise nothing happened such as I had anticipated. In the two weeks that the flood lasted those plants withstood it, apparently with some injury, but without in any way adapting themselves to those aquatic conditions, as I expected they would. It looks then as though certain plants had this method of standing submergence, and other plants had not. It is not possible for many plants to respond.

Professor Rogers' paper on "A Practical Method of Referring Units of Length to the Wave Length of Sodium Light," was then read by Professor Moler.

Professor Gage: The question may arise to the minds of some of you, What is the good of all this, anyway? I remember that back in 1882, at our meeting then in Elmira, Professor Rogers was talking about metrology, the same subject that is being discussed in this paper to-day. On that subject I believed most thoroughly that the way to get the length of anything was to buy a two-foot square in town at the hardware store, and measure it. That was the same opinion as Professor Rogers has told me (I know some of you have heard the story but some have not) was held by a carpenter whom he wanted to level up a table. For this purpose Professor Rogers gave him a good level—that is, what Professor Rogers would call a good level. The carpenter took it and tried to level up the table, but finally he gave it up, declaring that the thing was no good, it was bobbing around all the time. So he went home and got a 50-cent level, and had not the slightest trouble in getting the table level. I think you can see from this paper that if we are to get anything like absolute measurements, not merely approximate ones, the best skill of the most skillful men is necessary. We see from the paper the difficulties that Professor Rogers had—after he got his brass box, instead of its holding the air out it went in. All these difficulties arise constantly in experiments where we reach for something absolute. Of course where it is simply an approximation, a rough sort of approximation, there is no trouble; then we can use the ordinary carpenter's level or the two-foot measure that we buy at the store. But Professor Rogers is trying to do something very different; and he is, as many of us know, not exceeded in the world as a metrologist; he is recognized the world over, in his determination of exact measurements as a master. It is therefore with the greatest gratification that we have heard such a paper as that brought before us. To some of us perhaps there is a good deal that is new and strange in this, but this is exactly, as our worthy friend who welcomed us to the city and the university has suggested, this is just what we are after—getting at things that everybody does not know about, making additions to human knowledge.

Mr. Seaman: I suppose that to many people who have not thought upon the subject, the matter of weights and measures appears extremely simple. They suppose every foot rule is exactly like every other foot rule and every pound weight exactly like every other pound weight. Now I think that a little thought will show you how very far from a correct statement this would be in the ordinary methods of business. If you go back in the history of weights and measures you find that first of all people used as standards, parts of the human body. We have relics of that yet; the hand with which we measure horses is the width of the human hand; the unit of length known as a foot is derived originally from the human foot. If you go back to classic days, the land was measured by the number of hides of oxen that it took to cover it, and we find traces of that in our language at the present day. In England, in the time of Henry III., the grain, which we now know as an apothecary's measure, was actually a grain of wheat; and in order to establish some sort of uniformity they attempted to prescribe by law that the grain should be taken from the middle of the head of wheat, so that there might be a uniform size, because the grains at the end and bottom of the head were smaller than the other grains.*

Now it resulted from this attempt to furnish measures by natural objects that weights and measures were of all sorts. If you take a foot rule which you say is correct, and somebody else makes a foot rule by it and that one goes on to some other part of the country perhaps and somebody makes a foot rule by it, you will find that the little-accumulated errors will soon amount to an enormous variation from the standard. When the surveyor in the city of New York attempted to compare and verify some of the old surveys there, he found that it was impossible to do so, often within five or six inches of the original lines. In New York, where land is worth $250 a square foot, in many cases six inches amounts to an enormous value. The reason was that the old surveyors had never had any mode of verifying their chains. The way it was actually done is described in one of the old

*See Remington's Pharmacy, Art. Weights and Measures.

mathematical books, where the author prescribes that the surveyor shall go to a church at the time when the congregation is coming out, and call upon the first 16 people coming from the church to stand together heel and toe, and he shall measure the length of the line of feet for two rods; and thereby measured his chain. You can see that under such conditions as that accuracy of weights and measures was almost impossible. You could see in the wall of an old building in the city of New York a few years ago two iron staples or spikes driven in, in order that people might fit their yard-sticks between them to verify their length. Of course in a few years the spikes would become worn off so that the yard-sticks would become longer all the time.

I suppose hardly any of you know that in this country to-day there is but one, strictly speaking, legal system of weights and measures, and that that system is not the one in customary use. It is a very curious fact that the system of weights and measures which we derive from England has never been legally authorized by Congress. The only legislation upon the subject of weights and measures which Congress has ever made consists of two primary acts. One allowed the customs officers to use the English weights and measures for the purpose of ascertaining the customs to be paid to the government. The other act, which was passed in 1866, established the metric system as the legal system of weights and measures in this country. So we have the curious anomaly that the great majority of the people in this country are using a system of weights and measures which is, strictly speaking, not according to law. Now in connection with this I will state that when I first came to Washington 20 years ago and began to attend the meetings of scientific societies there, I rarely heard of anything but the English system of weights and measures. To-day you rarely hear anything but the metric system. Measurements of objects of natural history are mostly given in the metric system, so that that system has actually come into use by a large number of people in this country.

There is one other thought that I may mention—I am simply suggesting a few leading ideas. I stated that if you make a

measure and then compare another measure with that, and from the second make a third, etc., there is very soon introduced a great variation between the measures last made and the original one. There are but two standards of measure in the civilized world. The one is the metre of the archives in Paris; the other is the platinum yard made by order of Parliament in England, after the burning of the Houses of Parliament. If we take every measure in the world and refer them to those two standards, they will all be alike; and that is how this possibity of accumulating errors is avoided. It is in order to make accurate comparisons with these standards that all this work of Professor Rogers has been done; and it has been done with a perfection that, as Professor Gage has stated, has been equalled nowhere else in the world.

Prof. Hyatt then read his paper on "The Mouth Parts and Ovipositor of *Cicada septendecim*."

Mr. Seaman: One of the privileges of science is to dispel fictitious fears among people—the dread of that which is really harmless. In connection with Professor Hyatt's statement respecting the non-poisonous character of the *Cicada septendecim*, I am reminded of a very extended series of experiments carried out by Dr. Marx, a member of the Biological Society at Washington, in relation to the poisonous character of spiders, which are usually supposed to be dangerous creatures. He took a number of the spiders of the kinds which have been reported in the newspapers as having killed people, or having caused serious illness, or at least having produced disagreeable swellings; and he commenced by causing them to bite mice, and from mice he went to guinea pigs, and from guinea pigs he went to rabbits, and from rabbits to himself and other people. He tried many hundreds of experiments; he traced up, moreover, the histories of many of the newspaper statements, and he proved, beyond the shadow of doubt, that there is no spider belonging to this country, and perhaps in no part of the world—because he tried many of the large bird spiders of Brazil and other southern and tropical specimens—that there is no spider in the United States whose bite may be considered in any respect dangerous to life, or even

capable of producing in the majority of cases more than the swelling which arises from a common mosquito bite. There have been some cases—I think only two or three that he was able to run down—where illness, perhaps in one case death, was supposed to be connected with the bite of a spider; but a careful examination of the evidence shows that it was not, strictly speaking, due to the bite of the spider at all, but to that kind of inflammation and blood poisoning which might result from the scratch of a pin. Not but what spiders do have some poison, but it is so feeble and in such small quantity that it is absolutely without danger to human life.

Dr. Kingsbury read his paper on "The Lateral Line System of Sense Organs in Amphibia."

Professor Gage: I suppose we have all wondered in our youth when we caught fish what that streak was along the side of the body; and it has been the purpose of Dr. Kingsbury in giving us this paper not merely to speak of that in fishes but in the Amphibia. Ithaca, as you know, is peculiarly rich in Amphibia. There is a large variety of forms by which to test the theory, whether it is due simply to the water or to something else that we have that very interesting series, which Dr. Kingsbury has shown to us. He has been trying to get behind the appearances and find the reality, the true significance of things which on the face of them may not seem to have much significance—exactly the kind of work for which the society stands, it seems to me.

A paper on "A Comparison of methods of determining Hæmoglobin," by F. C. Busch and A. T. Kerr, Jr., was then read.

Professor Gage: I may begin the questioning by asking the reader if he will tell us how these tests correspond, for instance, to determining the number of blood corpuscles in the blood of the patient. Suppose we have a normal number of blood corpuscles—let us suppose 5,000,000 as the normal number. If now in your tests you found the hæmoglobin was too much or too little, would there be a corresponding increase or decrease of the blood corpuscles?

Mr. Busch: It has been found by observations made by a

number of observers that the hæmoglobin does not necessarily correspond to the number of corpuscles. It has been found, for instance, as regards specific gravity that it has some constant relation to the hæmoglobin, but does not depend at all upon the number of blood corpuscles.

Professor Gage: With a smaller number of blood corpuscles then you might have more hæmoglobin?

Mr. Busch: Yes, you might have more hæmoglobin.

Professor Gage: I believe there is at least an exceedingly marked relationship between the hæmoglobin and the number of blood corpuscles if the variation is exceedingly large. If the change in the number of blood corpuscles were smaller, say three or four hundred thousand, it might not be a very important factor. Dr. Moore, you have had expericnce in determining the amount of hæmoglobin and of blood copuscles; can you say a few words to us on the subject?

Dr. Moore: I think not. The work I have had experience in was not concerned with hæmoglobin. But the question I was just thinking about was this: In cases of anæmia, where you have a large loss of hæmoglobin, and in the organs such as the spleen, the liver and the kidneys a deposition of blood pigment, it seems to me that there must be a decided variation in the specific gravity of the blood, if, as is supposed, the specific gravity depends largely upon the amount of hæmoglobin. Now whether this hæmoglobin is free in the plasma or still confined in the corpuscles is a question I am not prepared to speak about specifically; but on general principles it seems that where blood corpuscles are destroyed in cases of anæmia, as I think has been found in certain of the lower animals—that in these cases the hæmoglobin is deposited and the specific gravity must vary. I think there must be a correspondence in the lower specific gravity of the blood and the diminution of the red corpuscles. What effect an increase of the white corpuscles would have on this relation of hæmoglobin I cannot say. But in those cases we have to deal with the fact that there is an unusual, an enormous increase in the leucocytes of various forms.

Professor Gage: We have here one who has made a great

study of the blood by aid of the micro-spectroscope. I would like to ask, Dr. White, if in your spectroscopic study you can make any correlation between experiments like those on the number of blood corpuscles and the indications that would be given by the spectroscope.

Dr. White: I do not think I can answer the question satisfactorily. I have noticed, however, that in examining specimens of blood some specimens have very little hæmoglobin, while others have a great deal. In some cases there is a very great difference in the corpuscles.

Just in that connection I would like to say one word in the way of asking a question, which I shall not pretend to answer myself. I have noticed in examining the blood of the dead in post-mortem examinations, that there are bodies, sometimes as small as red corpuscles, sometimes twice as large, that are colored almost as much as the red corpuscles. I have even seen these in regard to the living as well as in regard to the dead, and I am free to say I do not know what those bodies are.

Mr. Moody: It seems to me that while there is a definite relation or a somewhat definite relation, between the hæmoglobin and specific gravity, there are other things which enter into the specific gravity of the blood, that may cause as great errors as those arising in the experiments.

Dr. Moore: There is another question I would like to ask in connection with the specific gravity of the blood—the time of day of the experiments, and in what relation the taking of the blood stood to the taking of food by the patient? I should think if observations were made at different periods of the day in the same individual, there might be changes due to the different relation to the time of taking nourishment.

Mr. Busch: Our observations as a rule were made about midway between meals. At that time the blood is more likely to be in a normal condition. Some researches have been made by Mr. Jones, of Cambridge, showing that the specific gravity varied during the day under different conditions, as for instance, according to the amount of liquid taken in. I know that one German ob-

server says that upon taking one and one-half mugs of beer the specific gravity was diminished considerably.

Professor Gage: Have your investigations gone far enough to find out whether this is really a practically useful clinical method? I suppose that is a question we want to face and answer.

Mr. Busch: I will not say absolutely just at this time, because our observations are not completed yet; but there is no doubt that this specific gravity method also is liable to considerable error. We think that the other methods are liable to as much and perhaps more error. But there is an advantage in having a method which is not a color test. People's eyes vary so considerably. The idea is this—that if we can get a method in which the error is constant, then from day to day we can use it upon the same patient and observe his improvement.

Dr. Moore closing the discussion: I would like to say just a word or two in connection with this change in the blood after food. In my experience when we have taken the blood of dogs after death, if they were killed soon after they had been fed with milk or with a light sort of mush, we found as a result that the serum would contain such an enormous number of leucocytes, that when the serum was set the surface would be completely covered with a substance almost resembling cream in appearance—in structure if not in color, for it is white—a substance that really makes it useless for work. It seems to me that in taking blood in such large quantities as that you would have great variations. I should think that in determining the practical value of these investigations, they would have to be taken in connection with observation of the condition of the corpuscles, both red and white.

Professor C. H. Eigenmann then made a few remarks preliminary to his paper on "The history of the sex-cells from the time of segregation to sexual differentiation in *Cynotogaster*," and at the conclusion of the paper the society adjourned.

Wednesday afternoon, August 21, about two o'clock, the society assembled in the Physical Laboratory, at Franklin Hall,

and examined the ruling engine and comparator made by Wm. A. Rogers. From the laboratory we went to the Entomological department and inspected the excellent arrangement of insects made by Professor Comstock, and also the beautiful engravings made by Mrs. Comstock. Then the library was visited and those who wished ascended the tower to enjoy the view of the lake and country, while others examined the rare old books Professor Mr. Burr exhibited in the White Historical Library.

The beginning of Cornell University was the Morrill land grant of Congress to each State in 1862. After various struggles in the State legislature over the disposition of the grant to New York, during which Andrew D. White, of Syracuse, and Ezra Cornell, of Ithaca, became closely associated, on October 7, 1868, the university was formally opened. The distinguishing features of Cornell as a University are that it puts all truth on a level, and gives to the scientific or technical student the same standing as the student of literature and the classics. Hence the spirit of the whole institution is preëminently American and scientific as distinguished from those institutions where the dominating influences have been those of tradition and caste. Hence the young man who goes to Cornell receives a training that fits him for the real life of the present age in which he is to play a part. This year the university expects to welcome to her halls 2,000 students, including the women who find here precisely the same opportunities as the men, and how well they make use of them the pages of these proceedings bear witness. The library, while it is one of the most recent, is undoubtedly one of the most beautiful and interesting buildings on the grounds. Many of the members wandered off to the other buildings near by, including Sage college, the dormitory for women, adjoining which is the Botanical department, and also to the Young Men's Christian Association building and the chapel, both near by. Attached to the latter is a memorial chapel in which lie the bodies of Ezra Cornell and Mrs. Andrew D. White, whose forms sculptured in marble, rest on their marble couches as if they, too, were an imperishable part of the institution which is for one of

them one of the proudest monuments ever reared for any man.

In connection with this visit to the collections of the University the following letter from Dr. Wilder is appropriate for insertion here:

THE UNIVERSITY MUSEUM OF VERTEBRATES.

To Dr. W. H. Seaman, Secretary of the American Microscopical Society:

DEAR SIR— I regret my absence from Ithaca during the coming session of the society upon both personal and official grounds. It would give me pleasure to attend the sessions and meet the members.

As Curator of the Vertebrate Division of the Natural History collection of Cornell University, I should invite inspection of it. In so doing I should endeavor to state four points as follows: 1. The objects of the vertebrate museum. 2. The principles upon which it has been formed and maintained. 3. Its special merits. 4. Its chief deficiencies.

1. The Museum is primarily and mainly educational. That is, it is intended to illustrate the instruction of the classes in Physiology, Vertebrate Zoology, Neurology, and Embryology. At each lecture or practicum from 10 to 50 specimens are brought into the class room, and not only exhibited and explained, but left there for one or more days for fuller examination.

In accordance, however, with the doctrine that "none are so well fitted to impart knowledge as those who are engaged in reviewing its methods and extending its boundaries," the collections have been made the basis of numerous researches and publications by the staff of the anatomical department, and by specialists elsewhere to whom specimens have been loaned. There are also in rooms not open to the public considerable stores of material for investigation or for preparation as museum specimens.

On the other hand the wants of the public have not been ignored. The only division of the collection at present labeled with approximate explicitness comprises the animals of Tompkin county, whose names, life-histories and habits are of special interest to the farmers and fishermen of this vicinity. From the beginning the museum has been freely accessible during the whole of all week days. The janitor is often at work there or within call, and when funds have been available for the purpose a student-guide has been in attendance on Saturdays and at commencement. The museum is visited by schools from out of town and by excursion parties numbering hundreds at a time. So far as I am aware no officer or employe has ever received from visitors any pecuniary recompense for attendance upon them, for the explanation of specimens or for the exhibition of living animals.

2. The fundamental principle of the museum is the illustration of important facts and ideas by means of specimens carefully selected and well prepared. The wall cases at the north end of the main floor contain a synopsis of the vertebrate branch, and of the larger divisions commonly regarded as classes. As an example may be taken the two cases at the east

end of the series. Here are about 15 mammals fairly representing the range of difference in respect to form and mode of locomotion. Also some dissections displaying the structural features which are both constant throughout the class and peculiar thereto, e. g., the complete diaphragm, the left aortic arch, etc.

In a case at the west side of the same floor may be seen a series of a very different kind, viz., of the venomous vertebrates so for as obtainable.

Other series begun or contemplated are enumerated in my address on "Educational Museums of Vertebrates" in the American Association Proceedings for 1885: Abstract in Science, Vol. VI., 1885, pp, 222–24.

The most significant of these relate to the exposition of the general doctrine of evolution.

In the formation of these series mere number has not been considered, nor has the cost or the rarity of a specimen determined its real value. So far from taking all that is offered merely because it is cheap or altogether without cost, I have held that a wise economy would be practiced by paying for what was really needed, rather than in accepting less desirable objects as gifts. In an educational museum of vertebrates one flying squirrel is more desirable than a dozen other kinds. It would be wiser to pay ten dollars each for a *Sphenodon*, a *Protopterus* and an *Apteryx* than to receive as gifts a hundred other lizards, fishes or birds. Indeed, contrary to the prevailing idea that the curator of a museum is mainly a collector, I hold that one of his chief duties is to keep things out of it.

3. Among the desirable features of the museum may be enumerated the following:

a. The large number of embryos, brains and hearts of all classes.

b. The numerous well-preserved human cerebrums especially of educated persons.

c. The dissected preparations illustrating zoologic or physiologic facts and ideas. Some of the best of these were made by your distinguished President.

d. The association of such dissections of soft parts in the same case with the skins and skeletons of the same or allied forms.

e. The preservation of so many parts of one and the same individual. For example, of a kangaroo there are the stuffed skin, the mounted skeleton, the alinjected heart and brain, and some other viscera. This not only exhibits correlations of structure but also in case of the detection of an error in the identification of the skin, renders it possible to extend the rectification to all the other parts.

f. The designation of all parts of a single individual by one and the same number, and the use of the same number upon all notes, photographs and drawings relating thereto.

4. The most obvious defect of the collection is suggested by the last paragraph; few of the specimens bear labels conveying adequate information to the visitor. But it must be remembered that the collection is primarily for use in instruction and that although the tags may not be read easily from without the case, they can be when the specimens are in the

lecture room. Also, the number refers to an Accession Book and to notes that are accessible. Finally, among the reasons for the delay in labeling is the intention to supplement the conventional label by a somewhat full description and, in the case of anatomical preparations, by photographs, or diagrams or published figures. The attainment of this "triple alliance of object, drawing and description" requires much time and deliberation.

That many and serious gaps in the various series remain to be filled hardly needs admission. For these desiderata we must await opportunities, the thoughtfulness of our friends and the power of the trustees to supply the means of purchase and preparation.

The defects in the arrangement of the collection are due to three conditions:

a. The present necessity of accommodating upon the main floor certain ethnologic and archæologic collections which are incongruous and interrupt the natural series.

b. The limitations of space in the alcove cases and in those at the north end of the room. The malposition most to be deprecated is the association of the Dipnoans with the Ganoids, which constitutes in my mind, rank zoologic heresy. At present it is hardly to be avoided.

c. A recent interchange of cases with another department necessitated the transfer and storage of a considerable portion of the vertebrate series, and the rearrangement has not yet been effected. Hence some unoccupied spaces and some masses of undisplayed specimens. Should the American Microscopical Society again honor Ithaca by meeting there I trust the vertebrate division of the zoological collections may better represent my views and intentions regarding it.

Very respectfully yours,

BURT G. WILDER.

Siasconset, Nantucket Island, Aug. 17, '95.

Wednesday evening, about eight o'clock the society gathered in the Botanical lecture room to listen to the annual address of the President, Professor Simon H. Gage, on The Processes of Life Revealed by the Microscope; a Plea for Physiological Histology. The address begins this Volume.

NOTE.—Those who wish to know more of Ithaca and Cornell University, in addition to the catalogs of the institution, which can be had on application, will find a little pamphlet of Andrus and Church, "In and Out of Ithaca," very useful.

THURSDAY MORNING, *August 22.*

At 9.40 the society assembled, about 70 persons being present.

Professor Rowlee read his paper on "The Chlorophyll Bodies of *Chara Coronata*."

The President then appointed Dr. Kellicott and Mr. Kühne as a committee to audit the accounts of the Treasurer.

The following names were elected after free nominations to serve as a nominating committee to select officers for the ensuing year:

Messrs. W. W. Rowlee, D. S. Kellicott, W. H. Walmsley, G. S. Hopkins and Wm. C. Krauss.

Miss M. A. Nichols then read her paper on "Secondary Thickenings of the Root Stalks of Spathyema."

Professor Rowlee said when the paper was ended: It is due in justice to Miss Nichols to say that she began what she and I both believed to be a comparatively simple problem, and intended to keep it within very definite limits, and it was her intention when she undertook it to present it to this society. The discoveries that she made as soon as she began to investigate, however, led into such different lines that the paper has in it the possibility of a very important contribution to our knowledge of the monocotyledonous group. It is known probably to every one that the flowering plants are divided into two great groups, the monocotyledons and the dicotyledons, or, as they have been called, the exogens and the endogens. The endogens or monocotyledons were believed once not to have any secondary thickening, or, as the term implies, increase by internal growth. As Miss Nichols has said, DeBary found exceptions—others had found exceptions before him, and he summarized the exceptions; but they are comparatively few. There are always exceptions in natural history, you know, and it is fortunate if the exceptions do not come to be the rule. Neither of us had a notion that we should find secondary thickening when the work was begun. It was with a view to the seed, which Miss Nichols has described to us, that the study was made. Not only is the discovery of the secondary thickening new, but likewise the method of thickening is new; so that I feel we have

here a contribution to our knowledge of plants that is very important and which will be altogether creditable to our proceedings.

Dr. Hopkins: I would like to ask Miss Nichols if she has tried any experiment to see whether or not the crystals would be reformed after being dissolved out. If they would be, I should think it could be determined whether or not the irritation to the taste was caused by the crystals.

Miss Nichols: I did experiment with that in view, but the crystals would not re-form voluntarily, for I let the solutions stand for some time and examined them, but they did not re-form. Whether they could be made to do so or not I do not know.

Dr. Holbrooks' paper on a "Fourth Study of the Blood, showing the Relation of the Colorless Corpuscle to the Strength of the Constitution," was read by title only, and the society then listened to Mr. K. M. Wiegand on "Two Cases of Intercellular Spaces in Vegetable Embryos." The paper was discussed as follows:

Dr. Seaman: I would like to ask Mr. Wiegand as to the shape of the limiting cells on these canals, as regards their length. Are they longer than other cells, or not?

Mr. Wiegand: No sir, their length is about the same as their diameter.

Dr. Seaman: They are mere altered parenchyma cells?

Mr. Wiegand: Yes, altered a little in form. I might say that the arrangement of these spaces and of the cells around them remind one very much of the arrangement of cells around resin ducts in mature plants. The resin ducts in the pine family of course have a sheath of cells arranged in this way.

The paper of Dr. E. J. Durand, on the "Fruits of the Order of Umbelliferæ" was then read by title, also that of Dr. P. A. Fish, "The Action of Strong Currents of Electricity upon Nervous Tissue."

The society then listened to Mrs. Gage's paper on "The Morphology of the Brain of the Soft-shelled Turtle and the English Sparrow compared."

Discussion on Mrs. Gage's paper.

Dr. Krauss: I would like to ask if Mrs. Gage has made a

study of the cells of the cortex, especially of the frontal lobe, or that part of the brain of the turtle corresponding to the frontal lobe in man; and also what the development of those cells is, as compared with the development of the cells in the parietal lobe or the occipital lobe.

Mrs. Gage: To answer Dr. Krauss's question, I can simply say that thus far in my investigation I have confined my work to the general features of the brain, not at all to the histology; I do not know that I shall ever get to that point, there are so many problems in the way which must be met in order to homologize the parts and to be sure that the origin of the parts is the same, before a comparison can be made of the cells.

Professor Ward: I should like to ask, in the light of this investigation of the three series of brains, to what extent Mrs. Gage thinks the homology by fiber tracts can be followed out—are they characteristic of certain groups and homologous in the groups?

Mrs. Gage: Thus far only a few of these fiber tracts have been considered. If I could find the cell nidæ and the origin of them, and find that they are perfectly comparable, then I should feel that the fiber tracts which connect those nidæ are homologous. Otherwise I should feel that, simply because they go in the same general direction and follow the same general course, it is not necessary that they are homologous.

Professor Ward: Another word. To what extent have you followed this out and found homology?

Mrs. Gage: There are comparatively few of these which I have already done. The one which I have mentioned in connection with this investigation in these two forms, seems to be exactly comparable. There are certain tracts, like the great anterior and posterior bundles, about which there seems to be no doubt at all. There are these to be traced in all the forms of vertebrates and they seem to perform in general the same function and take the some direction; but some of these others, I should say, are much more difficult to trace. There are several others which are apparently, so far, quite easily determined. Some of the

more difficult ones, as those in the base of the cerebrum, I should say, would take a great deal of time to decide finally that they were homologous throughout the vertebrate series.

Dr. Humphrey: I was struck with regard to what Mrs. Gage says concerning the size of the optic nerves of these turtles, and I can state that these animals have a very acute sight. When lying upon the stones or logs along the course of our rivers, they will slide off when a person is sometimes at a distance of three or four hundred yards. But on the other hand I do not consider that their sense of hearing is very acute. A few weeks ago I was walking along the banks of one of our smaller rivers with a rifle. There were two turtles sitting on a log some sixty yards away. As I was behind some bushes they did not see me. I assisted one of them off the log with the rifle, but the other did not notice it at all, so I assisted him off in the same way. Often I have noticed that they pay very little attention to the crack of a gun, but they pay a great deal of attention to one's motion, and I believe sometimes that they notice the puff of smoke from a gun.

Mrs. Gage: How about the sense of smell or the sense of touch?

Dr. Humphrey: I do not know about that, I presume though, that it is very acute. I should infer that the sense of smell was acute from the fact that they live mostly on dead fish. I may say in regard to the habits of these, and of the large snapping turtles, those whose heads are perhaps three inches across, very large ones, I have found that their stomachs were filled with fresh-water algæ; while, as far as I have been able to discover, the soft-shelled turtles never take anything but flesh.

If Mrs. Gage will permit me I will say that I think a walk along a stream would cause her to change one expression. She said this animal led a sort of dreamy existence. It does in the winter time, but in the summer time I can see no analogy to a dreamy existence, unless because he is a nightmare to the fisherman.

Mrs. Gage: With reference to two points of which Mr. Humphrey spoke: In observing a shoft-shelled turtle it is very evident

that he keeps a very watchful eye upon you, but the only indication of motion whatever that he makes will be in the turning of the eyeball. You know he is looking at you from that one fact. There is no indication of nervousness on his part in any other way.

As to the sense of hearing I have observed in a jar, where I have studied them, that any loud sound near the jar will not produce any motion whatever in the turtle; while on shaking the jar even in the slightest degree they will move. I conclude from this that the sense of feeling was much more observable in this turtle than the sense of hearing.

Professor Eigenmann: I may add a word as to the habits of these turtles. We have had quite a number in the laboratory this summer. They have a habit of burrowing under the sand just sufficiently to cover themselves so that they cannot be seen. One which we were trying to photograph, when we went to get the plate, I think only across the table, was nicely covered when we got back so that we could hardly see it. One very large one that we got in a pen was missed after a few days. I thought he was literally in the soup: But he afterwards reappeared. These turtles lay their eggs in the sand. They lay quite a large number of eggs in a nest—the largest number that we secured this summer was thirty-two. The eggs are quite large—like good-sized marbles. They are white, but the upper surface of the egg is much whiter than the lower surface. I secured quite a number of their little ones, but have not so far been able to get any eggs to hatch. We still have a batch of eggs, however, and hope to rear some.

Mrs. Gage: Where do they lay their eggs; how near the water?

Professor Eigenmann: They lay the eggs within a few feet of the water's edge. Those we secured were within, I should say, ten feet of the edge of the water, right in the sand of one of the lakes. The easiest way to get them is to take a hoe and go to a likely place and just hoe over the whole of the region. You may strike a nest and then by a little more careful hoeing you can

get the eggs. We tried our fingers for quite a while, but found it was wearing our nails and otherwise not very agreeable. A hoe is the best thing—a spade is not very good.

There is no sign on the surface. You can find marks where the turtles have walked up, but on a beach a mile long the turtles had walked up all along it so that the tracks do not help you much. But once in a while you can find a particular track leading straight to a spot where there is a little depression in the sand, and there you can be pretty sure will be a nest. I may add that right beside the nest of thirty-two we found another with seventeen eggs. At any place where there was a point of land, by using a hoe we could secure from 150 to 200 of the eggs in a short time, perhaps hardly fifteen minutes.

Mrs. Gage: It seemed to me these turtles had the most self-restraint of any animal I have ever watched. I sat by a tank watching one for ten hours one day. I think Mr. Humphrey would have been convinced that my conclusion about their dreamy existence was correct. That turtle stayed at the bottom of the tank for ten hours, and never once moved in that time. He had filled up his lungs with oxygen and was breathing down there by means of his gills situated in the throat.

Dr. V. A. Moore then read his paper on "The Flagella of Motile Bacteria."

The President said: It is one of the sources of congratulation for societies like this that we have problems presented to us for investigation, and that as naturalists or investigators in science, we have never to weep as Alexander did for new worlds to conquer.

May I ask Dr. Moore if it is the supposition that these flagella move, or have they been actually seen to move.

Dr. Moore: Some of the earlier observers have published the statement that they have seen these flagella in the larger forms, move in stagnant water. I will say that in examining a water bacillus not long ago in a liquid culture, in the hanging-drop preparation, as it is technically called, I saw these flagella, a mass of them, rolling and twisting about, perhaps by means of

currents in the liquid; but I could not see them attached to the bacilli themselves. But they have been observed and described. Dallinger and Drysdale watched a bacillus, as they called it at that time, whatever it was I do not know, one of those organisms—for ten or twelve hours, first one and then the other, until finally they saw a curious vertical appearence of the liquid at the poles of the organism; they said they saw this fine hair-like projection move. Those statements have been made, and I do not doubt that with proper optical appliances they could be seen in the larger forms.

Dr. White: I should like to make a statement in regard to some of these bacteria that I think has not yet been published. It may be of some interest on just this question of the motility. Dr. Foote, of Yale, has prepared some cultures, both in plate cultures and stab cultures in test tubes, and he asked me to photograph them. Now these stab cultures in the test tubes in glycerine after three days' culture, the *Coli communis* appears as a line or thread generally where the track of the wire was inserted; while a simple culture of the typhoid bacillus shows the culture generally following the track of the needle, but also spread out laterally so that it is funnel-shaped. At the top of the test tube it will be very wide, perhaps half-an-inch wide, and tapering down to the bottom. To my mind this different appearance is one kind of proof that these bacilli move and travel in the gelatin. Also in the plate cultures of the *Coli communis*, which is generally considered to have very few flagella compared with the typhoid bacillus—the colonies after three days' culture in this same material are clearly defined at the edges, sharply defined as little dots; while the similiar cultures of the typhoid bacillus have a distinct areola around them, about as wide as the colony itself. If I had known that this paper was coming on this morning I would have brought photographs to illustrate this point.

Dr. Krauss: Just to show the importance of the flagella in making a comparative examination of the bacilli—about a year and a half ago we had an outbreak of typhoid fever in Buffalo. In

trying to detect the source we examined the water of different localities. We suspected that the water commissioners were using a source of water that had previously been used in Buffalo, but not for the past few years. Now as to this old source of water supply we know that certain parts of the city drain into it. It was during a north-east storm when the water supply was rather low that they used this old water supply. After a certain period of incubation the typhoid fever broke out in all parts of the city. We examined the bacilli in the old water supply and found almost as much bacilli as water. In making comparative tests we found that the typhoid bacillus was present and also other bacilli resembling the typhoid so closely that it was almost impossible to tell which was the typhoid and which was the other. It was only on staining for flagella that we were able to state positively that the typhoid was present and undoubtedly caused all the trouble.

Now as far as the function of the flagella is concerned, it occurs to me, why cannot these flagella be weapons of defense for the bacilli in their warfare against the phagocytes. We know that almost all living bodies are supplied with weapons of defense of some kind or other, and these may possibly be the creatures' way of fighting off the phagocytes as long as possible. The power of resistance to disease in different persons is very different, as we know, and certainly the resistance to antiseptic action of some of these forms may be due perhaps to the flagella.

Professor H. B. Ward then read two papers, one on the "Primitive Source of Food Supply in the Great Lakes," the other on "Some Experiments in Methods of Plankton Measurements.

Discussion on Professor Ward's papers.

Professor Rowlee: I would like to ask Dr. Ward two questions. He mentioned three conditions which determined the presence of organisms more abundantly in the upper six feet of the water than elsewhere. Those three conditions do not include one that it occured to me might very considerably affect their presence there, and that is the amount of air or oxygen that is in solution in the water. Is it not true that there is much more of that generally near the surface than in the deeper water?

Professor Ward: That matter has never, so far as I know, been investigated in fresh water. In salt water, Agassiz says in "The Cruise of the Blake" that experiments made at a depth of some one hundred fathoms show nearly the same amount of oxygen as near the surface.

Dr. Eigenmann: I think that it has been shown that there is more oxygen further down than on the surface.

Professor Rowlee: The other question was in regard to shoals of fish visiting the mouths of rivers and streams. In Lake Ontario the best fishing grounds are near the mouths of rivers; and whenever there were great quantities of white fish taken by nets they were usually, I do not know but universally, drawn where streams flowed into the lake.

Professor Eigenmann: Was that in the fall of the year?

Professor Rowlee: I think they drew them all summer, at all times of the season. It occurred to me that that might be explained by some unevenness in the supply of food.

Professor Ward: I do not know how that is on Lake Ontario. The best fishing grounds in the northern lakes are not in such situations. The best white-fish ground in the vicinity of Charlevoix is on the west side of Beaver Island, which is a straight sandy beach, so far as I remember, entirely without a break of the smallest size. There is this much to be said with reference to the white fish. There was evidence, I think, discovered last summer to lead me to say that I believe the true white fish is a bottom feeder, and in that respect differs from all the other species of white fish in the lakes.

Dr. Fell: I wish to make a remark on some observations I made in Lake Erie last summer. I noticed great quantities of small fish cast up on the shore from Crystal Beach west and also from there east, and I presume along the whole shore of Lake Erie. There were millions of fish two or three inches long thrown up on the sand and destroyed. Whether these were products of the fish culturists or not I cannot say, but it seemed to indicate that there was some great mistake made in supplying the lakes with fish spawn, if such was the case.

But I have not seen any reference to this great destruction of small fish before. I have not noticed it this year, but last year the dead fish were plentiful, so that to walk along the shore of the lake was very unpleasant indeed. You could not step without putting your foot on two or three of them.

As to the question of food supply I recollect some years ago we made an estimate of the character and quantity of diatoms passing Niagara River; and we ascertained that some tons of diatoms passed down there every day. Undoubtedly diatoms are a great original source of food supply for the lakes.

Professor Eigenmann: Professor Ward's paper was among those which I especially was anxious to hear. The importance of such stations has long suggested itself to me. I believe that sometimes there is contemporaneous evolution in different regions, and I think that something of that kind has occurred out our way in regard to this matter of studying the conditions of fish life. Illinois has established a station, Michigan has established a station, Indiana has established one this year, and Ohio is going to establish one next year, to observe just such things as Professor Ward has been telling us about.

I have made some observation on the plankton this summer, but that was only as one of the elements of the environment of the fish. We have attacked the fish problem in a little different way. We have established a station on the continental divide between the St. Lawrence and Mississippi basins, at a place where within five or six miles we can get lakes of practically the same dimensions, one belonging to one system and the other to the other system. More than that, within a short distance we get lakes belonging to Lake Erie, and other lakes belonging to Lake Michigan.

We are attempting there to study the environment of the fish and the variation of the fish—the environment simply to give us the unit for studying variation. We are catching as many fishes as we think will suffice to get at the entire variation of a given species in the water of these lakes—so many that if we caught all the rest of them it would not make any difference in our measurements. For instance, we have of one little fish, caught in

one of the lakes, something like 700 specimens now, in which we will measure the variation. We have caught about as many in a neighboring lake to measure the variation there. We are measuring the environment and the variation in one place to get a unit in order to measure the influence of change of environment upon the same fishes in these different lakes to be found around our present station.

To return to the turtle question for just a moment. Most of these lakes have been raised in quite recent years by the building of dams, and that has flooded some of the lowlands that were covered with trees; the trees were afterward chopped down so that we have along some of the margins many stumps. These stumps have rotted so that there is a depression in the center. These have been seized upon by the turtles as breeding places. The little turtles crawl up these stumps and lay their eggs in them. We have got as many as 362 turtle eggs, I believe, out of one of these little places. Rotten logs are frequently full of these eggs. There are places where in a cow track going across a wheat field every depression that the cow made in walking while the ground was soft contains turtles' eggs.

Professor Kellicott: One or two questions have been raised which I wish barely to mention. In regard to the distribution of the plankton, I may say something in regard to the time which it takes for a floating object to move from Lake Huron, say, to the foot of Lake Erie. Those who have worked at Buffalo are acquainted, I think, with the fact, that pine pollen is found floating in the water past the city from the first of January to the first of March. Early in the summer while the pine pollen is being thrown into the lake there is none at all. After the date I have mentioned there is practically none at all. Of course a storm will sometimes stir it up from the bottom so that we find it in small quantities throughout the year. But during the time from January to March a great deal of pine pollen comes through the water to Buffalo. This would seem to show that the forces of distribution are ample, even if these minute forms do move so slowly of themselves.

In regard to the diatom *Stephanodiscus Niagaræ* it occurred also in the winter, beginning earlier than I have mentioned, and late in the fall, in great quantities, and living and continuing for two or three months, when they ceased. Just where that diatom occurs most luxuriantly I am not aware. It has never been found, I think, in shallow lakes, except Hemlock Lake. That fact would also have, I think, some bearing on this question. It must be, I think, an inhabitant of deep water.

Mr. Ward speaks of crustacea being the food of a certain fish. Perhaps he is aware that mysis sometimes makes its way into the water supply at Buffalo. We have taken it there, always when the ice is going out in the spring.

In regard to the weighing of the plankton, we estimate the quantity by weight. I suppose we were rather crude because we did not dry the material absolutely. We dried it until the water was just gone, and tried to estimate the exact amount of moisture in that way. That method, when we weighed a large quantity, could cause, I think, only a slight variation. It gave only a relative result, but I think that relative result was pretty good. The method by ash I have some doubts about, because floating in the waters of the lakes there is always a large quantity of inorganic matter. If this matter were constant it could be disregarded, but it is very inconstant.

Professor Ward: I think Professor Kellicott has a little misunderstood my statement. We used the ash precisely for that reason, to get rid of that inorganic element. There is always, as we found by microscopic examination, some sand floating in the water. We can find individual grains of sand which in weighing make considerable differences. This is especially important when only a small amount of material is taken in a haul. For instance in one haul we have taken only .95 c. c. of material. In estimating that, a small quantity of sand makes a great difference in the weight. But by taking the weight air dried and taking the weight of the ash and subtracting it, we eliminate the inorganic substance in the water, eliminate everything except what is organic.

THURSDAY AFTERNOON.

On Thursday afternoon the society with their friends were given an excursion from the Campus to the Lake by the street car company and upon the Lake by the citizens of Ithaca. This excursion was most enjoyable in every respect. The day was perfect, and through the kindness of Professor Tarr and Professor S. G. Williams, of the university, the interesting geological features of the lake basin were pointed out to the members.

FRIDAY MORNING, *August 23*.

The society assembled at 10 o'clock in the McGraw building, about 90 persons being present. The nominating committee made its report as follows:

Officers of the society for 1895–96:

For President, Dr. A. Clifford Mercer, of Syracuse, N. Y.

For Vice-Presidents, Edward Pennock, of Philadelphia, Pa.; Miss V. A. Latham, of Chicago, Ill.

For Secretary, Dr. William C. Krauss, of Buffalo, N. Y.

For Treasurer, Magnus Pflaum, of Pittsburg, Pa.

For members of the Executive Committee, Professor C. H. Eigenmann, of Bloomington, Ind.; Herman Schrenk, of St. Louis, Mo., and Miss M. A. Booth, of Longmeadow, Mass.

Upon motion, the secretary was directed to cast a ballot for the nominees as reported by the committee, and they were duly elected.

Professor Conser then read his papers on "Cocaine in the Study of Pond Life," and on "Paraffin and Collodion Imbedding."

Discussion:

Professor Eigenmann: I might add a word or two as to the use of cocaine. I have found it very useful in studying the habits of fishes. In one case especially that I remember, where I secured only one little larva with a net in sea-fishing, it was very valuable and I wanted to make as many sketches as possible at successive stages; but it would not hold still, and of course I did not want to kill it. So by judiciously adding a little of the cocaine, that the druggist would let me have, I succeeded in keeping it still

quite long enough to draw; then by rapidly changing the sea-water I succeeded in having it live another day. By repeating the operation in that way I got four or five sketches of it. It thus proved very successful.

Dr. Mercer: I am not very familiar with these methods as I am not in the habit of doing much practical work now, but I saw a "wrinkle" a year ago which I suppose some worker might be glad to hear of. It was in Mr. Andrew Pringle's laboratory on the other side of the water. He was using the paraffin method almost exclusively. He placed his specimen, when he wished to saturate it with paraffin, in a chamber which he could exhaust. The temperature of the chamber was regulated by a thermostat. In a very short time, in comparison with the usual methods, the paraffin had saturated the specimen, by reducing the atmospheric pressure around the material. As a means of saving time I dare say the busy worker will appreciate it.

Professor Conser: I have used the method of placing the object under an air pump, which has reduced the time considerably. It is a very practical method, though it is somewhat complicated and troublesome.

Dr. Mercer: I might add that this was done in the case I mentioned in a chamber which could be kept at any desired temperature, which is a little addition to the air pump.

The President: I would like to ask Professor Conser why he uses celloidin, which costs $1.25 an ounce, when gun-cotton can be got for 25 or 20 cents an ounce; and I feel confident that any one using it will find he gets just as good sections. This gun-cotton is made in America, the celloidin in Germany. I had occasion one time to buy eight or nine ounces of the German, and I kept it very carefully and only used it as I thought we could afford it, and in a little while it would not work at all. I tried it and found it exceedingly acid. In despair I went down town and bought an ounce of gun-cotton for 25 cents, and things commenced to work. After that I discarded the expensive for the cheap.

Professor Ward: May I ask the President if he has ever

had any difficulty in getting a quality of gun-cotton which is free from little fibrous foreign bodies? All I have been able to find was quite dirty in that way.

The President: What I do in that case is to make up the collodion and let it stand a little while; and all that settles to the bottom, so that all the rest is perfectly clear. Or I filter it, but that is not so good a way. If allowed to settle the upper part is very excellent.

Professor Eigenmann: Is this gun-cotton precisely the same thing as celloidin?

Professor Gage: Yes.

Professor Conser: It won't go off?

The President: Yes, they will both go off if you want them to. They are both of them nitro-cellulose, I believe.

Professor Conser: I want to add a word in regard to the keeping qualities of celloidin. It must not be kept in air-tight chambers. It can not be kept in glass-stoppered bottles, for example. There will be some decomposition in that case, especially if kept at the ordinary warmth of the laboratory. Though I have used gun-cotton to a very small extent, I have preferred the celloidin, from the fact that it was easier to dissolve, and more ready at any time for operation. My experience with gun-cotton, however, has not been very much; it has only been experimental.

Dr. Krauss then read a paper on "Formalin as a Hardening Agent for Nerve Tissues," and Dr. P. A. Fish one on "The use of Formalin in Neurology."

Discussion:

The President: Probably no reagent has been discovered and applied to anatomical preparations, to microscopic work, which is going to do so much, help us so much, as this one formol, and every detailed account of experiments will be therefore of the highest value to serve as a guide. I am particularly glad that these two papers have come before the society to give us information, and I hope that those among the members who have made experiments will tell us of their experience. We then can put all this testimony together and go on more successfully in

the future. I hope Professor Kellicott, who has experimented with this substance, will tell us the results he has obtained.

Professor Kellicott: I have worked for about a year in preserving by this method and I must say that the result agrees so closely with the results given in the papers that it seems hardly worth while to take any time in stating my experience. I have used the formalin in various solutions, and on a great variety of tissues, in preparing them for museum and anatomical work, and the results have been most excellent. I think in our laboratory we are going to harden them by one or two changes, then transfer them to about half alcohol and half water, for preservation. This method seems to give us better results than any other we have tried, at least. Formalin hardens animal or other tissues very quickly, and without much shrinkage, as has been said; and when transferred to alcohol they remain without shrinking, provided it is not too strong. The specimen will be preserved without deposition of coloring matter or sediments, so that really sometimes the formol does not need to be filtered or changed. It preserves the specimen clear and transparent, perfect in every way. I have used it in preserving the brains of large animals as well as small ones, for museum purposes, and have found nothing like it. It requires less material, less time, and only a quarter of the pains. You can put your specimen into the solution and go off and leave it, and when you come back it will be all right, just as you left it.

We have used it also for histological purposes to a limited extent. In that regard we must work further before we are ready to state any general conclusions.

As to the odor, I found that some students are unable to use the formalin on that account without great annoyance to themselves. There was one student in my laboratory who could not use at all a specimen that had been preserved in this way, the irritation was so great.

I have had experience, I think, similiar to your own, in preparing anatomical materials for the ordinary dissecting work of the laboratory. I think in that regard it is extremely valuable.

When the tissues are washed out in the usual way with a salt solution and then injected with a formalin solution of two to four per cent.—I mean by volume—the animal will remain, if kept cool, ready for use without any change or shrinkage. We are using it in that way constantly, and it is so valuable that I want to recommend it to every one who has to prepare animals for the dissecting laboratory.

I have had some experience in attemping to stain in bulk before the tissue is hardened, with carmine, for example, and I have had excellent results. When I have had time to experiment so as to ascertain the right time and temperature for the work, I am sure it will be of the highest value.*

The President: May I ask the writer of the first paper what he had reference to in giving us the percentages? Here is going to come in a real difficulty in the use of formol. It is a forty-per-cent. solution in water, and when we say we use a two-per-cent. or a one-per-cent. or a four-per-cent. solution, the question immediately arises—it did in my own mind when I came to use it—do we mean that we take 97 or 98 c. c. of water and 2 or 3 c. c. of this formalin solution, or do we mean that we take 95 c. c. of water and 5 c. c. of the formalin solution, that is, to get a truly two-per-cent. solution of formol. In some of the papers that I have heard read the statement has not been distinct as to whether it was by volume, as Professor Kellicott spoke of, where you would use 2 c. c. of the forty-per-cent. solution formalin and ninety-eight per cent. of water, or whether the absolute amount of formol was meant. Dr. Krauss, what did you use?

Dr. Krauss: I used two per cent. by volume.

The President: That would be then really an eight-tenths-per-cent. solution of the formol. I hope that will always be stated so that there will be no mistake. The chances are if it is not stated that some one who has had training as a chemist may use a really two-per-cent. solution of the formol and that might be too strong.

*Professor Kellicott has written a detailed account of his experience with formol, and the secretary has added it to the papers read at the meeting.

Professor Ward: I want to make a few statements in regard to this matter, because my experience, with perhaps a different class of objects, is so absolutely different from what has been reported this morning. If it were only on the basis of my personal experience, I should hesitate to emphasize the point as strongly as I want to do. I know, however, by personal correspondence of experiments carried on at the University of Michigan, at Harvard, and at the Newport Marine Laboratory, and they all agree with my own, that for certain purposes formol is not only useless, but positively bad, worse than useless. Some of you may remember noticing in the January *Naturalist*, if I remember correctly, quite an extensive article on the use of formol, by a gentleman working in Professor Kingsley's laboratory. He was very enthusiastic in praise of formol. By personal correspondence with one of our faculty at Lincoln, I have learned that his preparations have all spoiled since that time. He recommended formol very highly in the article on the basis of three months' experience. The same thing was true of my own experiments, the same thing was true of experiments at the Newport Laboratory and at Harvard. The preparations are good for a limited time, but histologically at least they are useless after that limited time. How long that time is has not yet, perhaps, been determined. That of course is where the formol is used as a preservative agent, and I think we ought to distinguish sharply between its use as a hardening agent and as a preservative. As a preservative I am convinced that for invertebrates at any rate it is very bad. It gives distorted and incorrect histological features every time. There may be a difference in the action on vertebrate and invertebrate tissues, and I have not experimented with it on vertebrates except for gross anatomy, but for preservation of the invertebrates it is very bad.

In one of the papers, I think that of Dr. Krauss, I noticed a little sentence that may have something to do with this. He said that in some of his preparations the nerve cells seemed to be slightly swollen, and the nucleus stained very deeply. Is that a beginning of a change opposed to the ordinary shrinking, a

change by enlargement of the cell which, if it goes on long enough, will result in serious damage to the tissue? Where we are not concerned with a simple diagnosis of a certain kind of cell, but are desirous of securing a correct histological image, or making a careful physiological study of that structure, it is clear that we must have some reagent which will preserve permanently and as nearly as may be the actual character of the cell, neither swollen nor shrunk, but in precisely its actual normal condition.

The statement quoted in one of these papers that the coloring matter is preserved, I can distinctly negative for as widely-separated classes of invertebrates as the fresh water mites, crustacea, worms and hydroids. In none of these is the color preserved ordinarily beyond a few days. The color of planarian worms, for instance, both fresh water and marine, while comparatively well preserved up to a month, is entirely lost within a period after that, so that at four months the color is entirely gone, and at that time the worms are in poor condition.

In addition to these points let me say that formol is highly volatile, and unless the bottles be very carefully closed the solution deteriorates rapidly. So for instance we found that at the end of four months an ordinary homœopathic vial corked as closely as the best quality of A A * corks could close it, did not contain a recognizable trace of formol. The specimens had entirely deteriorated, and that not in a single vial nor due to accident, but in a whole series. Whether this can be avoided by the use of vaseline on the corks, I do not know. Certainly under ordinary conditions formol will disappear very rapidly.

Professor Eigenmann: The last point that Dr. Ward mentions is a serious one, since so far as I know now we have no means of testing the exact per cent. of formol that may be in the water; so that after we have used a solution once for hardening, for instance, it must be thrown away or else we have to deal with a solution whose per cent. we do not know. I have tried the formol—there seems to be a perfect epidemic of formol experiments in the country—I have tried it on various things. I tried it on plankton and the plankton would not settle, so at least in

measuring it is useless. We tried it on crayfish and all of them spoiled, but I think that could be avoided by using a little stronger solution. By injecting the crayfish with a strong solution and then putting it in a weaker, it certainly becomes a beautiful object, and looks as if it were going to crawl off. Frogs can be preserved so that they look very much alive indeed. On fishes I have tried it and the color utterly disappears. I was in hopes I could preserve the beautiful colors of our fresh-water fishes—and they are certainly the most beautifully colored creatures alive. The formol is also, I think, valuable in injecting into the muscles of larger fishes that are to be preserved. Alcohol does not penetrate quickly enough to preserve them. At Wood's Holl last summer I put a two-per-cent. solution of formalin in a small vial, and crowded as many little fish into it as I could, fairly jammed them in ; and I must say that a year afterward the fishes are just as hard, just as firm, firmer in fact than when I put them in. I tried it on tad-poles. They are very hard to preserve. I tried a two-per-cent. solution of formalin, in thirty-five per cent. alcohol, fifty per cent. alcohol, etc., and also in simple formalin, and the formalin gave the best results. The tad-poles are so firm that if you take them by the tail and flop them they stand rigid, while, of course, in alcohol preparations they are flabby and very disagreeable.

I would suggest that the difficulty regarding the percentage could be avoided by using the name formol for the pure formol, and formalin for the forty-per-cent. solution. I myself have used it that way.

Miss Clara Harrison : I have tried some experiments with formalin. Some three years ago, perhaps a little more than that, I had for sometime been trying to find some solution that would preserve the color of fruits and flowers. I heard of formalin, and as I could find no literature, that is, very meager literature, my experiments with it were rather of a shot-gun kind. I began with five per cent. of the forty-per-cent. solution, and went down to one per cent. I tried it on the Orchidaceæ, and I am sure their coloring is quite as fine as that of our fishes. I found that the yellows and the purples kept for a long time. I tried, as I

remember now, one of the Mexican Orchids and it kept clear and perfect for about four months. I might say that I put a grain or two of corrosive sublimate into the solution. For about four months these kept perfectly in a little test tube corked with a rubber cork. I was delighted; and one day I thought I would take it down to the White House, where I had obtained the specimen, and compare it with a fresh specimen. I found the color identical. But the next morning I was very much disappointed to find the plant white and the solution a beautiful red. So my experience is identical with that of Dr. Ward. It does well for a short time, up to about four months, in a one-per-cent. solution; but after that I would not like to answer for it, that is, on fruits and flowers.

The President: It seems to me that this discussion has brought out the advantage of presenting a subject to a society of people who are interested in the same thing, then by comparing notes it is possible to see wherein the thing is useful. We have found, I suppose, all of us, that there is no panacea for anything. Formalin is certainly admirable for certain things; but that does not say it is going to be admirable for everything.

A paper on "New Points in Photo-micrographs and Cameras" was then read by Mr. Walmsley.

The President: It was in 1882—before I had ever thought of such a thing as making photographs with the microscope—that I had the pleasure of hearing Mr. Walmsley speak upon this subject. I think the society is to be congratulated that we have with us, so to speak, a direct lineal descendant of Woodward. As he has said, the work that Mr. Woodward did showed the world what it was possible to do in the delineation of minute things by photo-micrography.

Miss Latham then read a paper on "The Question of the Correct Naming and Use of Micro-Reagents."

The President: It is not often that we have so vigorous a presentation of a thing, and I do not wonder that Dr. Latham speaks with some feeling, because we have all gone through the same experiences, more or less, that she has.

Mr. Clark Bell then presented a communication on "An Inquiry Concerning the Possibility of Distinguishing Arsenic from Different Packages by the Microscope."

It is due to Dr. White that this question has been presented to the Medico-legal Congress, and I thought it might be well to ask the microscopists of this country if they would give us some aid upon the subject. Any one who wishes to investigate the question can address me and I will respond by mail and send any literature upon the subject. The question arises out of the celebrated murder trial in Connecticut. I will ask Dr. White to explain the details of the case.

Dr. White: In the famous Hayden trial it was proved that Mr. Hayden had purchased an ounce of arsenic in Middletown, and it was claimed by the State that he had administered this to Mary Stannard. He claimed that he had put the arsenic in his barn, and an ounce of arsenic was found in a tin box in the barn. The State asked whether it was possible to determine whether that ounce of arsenic came from Middletown or not. The arsenic which was found was examined with a microscope and was found to consist of very many crystals with bright reflecting faces, whereas the arsenic in the jar in Middletown had dull, leaden faces. The arsenic found in the stomach also had dull leaden faces. The question was whether these two parcels of arsenic could be distinguished. Many parcels of arsenic were bought from many different firms, to find, if possible, where this arsenic came from. One expert in Chicago has stated that in his opinion this dull, leaden color of the arsenic occurs by deoxidation, but chemists tell us that cannot be. It is probably due to some peculiarity in the grinding. What chemists know as glass arsenic, when ground, will not have transparent reflecting faces. The large crystals found in some places where arsenic is made, if ground up, will also show rough faces; whereas the arsenic found close to the furnace, if ground, will still show many small bright faces.

If in this case the arsenic found in the barn was just the same that Mr. Hayden bought in Middletown, it was clear that he might

be innocent. If it was from some other source it might be that he was guilty.

This idea of distinguishing different parcels of arsenic by the microscope was first brought to my attention in Medical Jurisprudence some thirty or forty years ago. In 1860 a lot of crystals were made for me by the chemist in Middletown. I have had some of them preserved ever since; and I think some of them undergo a process of pitting. It is a question whether if kept for some time they lose weight and form pits in the faces.

The paper by Dr. Krauss on "A New Way of Marking Objectives" was then read. The two following papers were read by title only, viz.: "Demonstration of Histological Preparations by the Projection Microscope" by Drs. Krauss and Mallonee, and "Improvements in the Collodion Method" by Professor S. H. Gage.

Dr. Mercer exhibited an improvement on the Syracuse solid watch glass, having a peculiar shape so that they may be piled together without slipping.

Mr. Pflaum exhibited a metal block for centering slides which he found very convenient. It is figured in his article in this volume of the proceedings.

The society then adjourned to 3 P. M.

BUSINESS MEETING.

FRIDAY, *August 23d.*

The meeting was called to order at 3 P. M. in the McGraw hall.

The Secretary: The Constitution requires that amendments shall not be made except on a year's notice. Article II provides that membership may be acquired by application in writing, recommendation by two members of the executive committee, and election by the society. Notice is given that next year an amendment will be proposed striking out the words from "nomination" to "society" inclusive, and so changing it that it shall read "and election by the executive committee."

The purpose of this is to prevent the long delay, often nearly a year, before persons applying between the meetings could become members, although in practice they were furnished the proceedings like regular members.

The President: This notice of amendment then has been duly given, and next year at our meeting it will be acted upon and decided.

The Secretary: A proposition has been submitted that there be two classes of members, or two conditions of membership for life, which would add to our income. The first is that persons or organizations desiring to obtain the proceedings, by paying $50 would be considered as subscribing members, without any other rights of membership; while those paying $100 would be full-life members, and entitled to take part in the proceedings of the society.

The President: This will be considered as due notice that this amendment will be brought up for consideration and final action next year. The proposition concerns membership and requires an amendment to the Constitution.

The President: We have difficulty in storing the property of the society, especially the plates. The drawings for these are furnished by the writers, and they are made at the society's expense. I propose that after an article has been published the plates shall go to the writer.

I think the plates will do a great deal more good in that way than in the hands of the society, where they are practically a burden. Not one plate in fifty will ever be used again by the society. A person interested enough to make the plate would likely in future work find real use and convenience in having these plates to show some other phase of the subject.

Professor Ward: Is this to be retroactive, or simply to deal with the future?

The President: As the stock is a burden to us, it seems that it should refer also to the past as far as possible.

Professor Ward: I make a motion then to the effect that the plates now in the possession of the society, or those to be used

in future publications, be given to the authors of the papers in which they are used.

The society is supposed to get enough out of them to pay for making them, otherwise they would never have been made. Now they are so much dead timber on hand.

The Secretary: The society expects to keep some of its publications, and there must be a place to store them. It is very unfortunate that there cannot be a permanent place of deposit for all our property. It is not merely a question of the plates, but of back sets and also of donations of the proceedings of other socities, of which we get a good many and would get many more if we had a place to keep them permanently. There are also some preparations and a number of other things. A great deal more has been lost than we now have on hand. The society is expected to be national and to be permanent. The individual is transitory, and his heirs do not take proper care of such property as plates. Plates that have been prepared by students, say in Cornell University, might be returned and they would be kept properly by the University; but that is not so with outside contributors. To return to them will be throwing them away. The society has paid for them, and they ought to remain its property and a place provided for them. They take up actually little space—all could be piled under one table. With the exception of the plates for one number, all the plates made before I became secretary have been practically lost.

The President: That is a ground on which I proposed that they should be lost by the individual and not by the society. Science is changing and the plates are of little value after a short time, in most cases. Those making the plates may be able to use them once or twice more before they have lost their value.

Professor Rowlee: There seems to be the larger question of a permanent home involved. May it not be well to leave the matter to a committee, of which the treasurer shall be chairman, to provide a place for permanent housing of our property? I am sure that the plates prepared in the botanical laboratory here

would be of more use to us in future publications than they could be to the society. They may be used to illustrate some new phase on the same general line.

The Secretary: It was stated in a previous meeting that the Buffalo Academy of Arts and Sciences, in whose building the Buffalo Microscopical Club has its home, would afford us a place. I favor a committee to consider that subject. There is much interest in microscopy at Buffalo, and it is conveniently situated. I should think either there or Cornell University the best place.

Mr. Young: It is early in our history to begin giving away things. We may think we must give away other things. I think other members should be consulted, and move that the matter be deferred to the next meeting. In the meantime I favor a committee on the subject of a permanent home.

Dr. Mercer: I move that the whole matter be referred to a committee of three or five, to consider also the larger question of a home for the society, as well as its property. There we could have a room, collect our books, etc., and meet there not necessarily always, but when we had no other invitations.

This motion was seconded.

Mr. Milnor: If Pittsburg were made such a home I can guarantee a place for storage that would be fire-proof.

Dr. Mercer: Dr. Krauss expressed the same thought to-day as to Buffalo. The President stated the question to be on the amendment to appoint a committee of three, as made by Dr. Mercer.

Professor Rowlee: We must notice that we are about to transfer all this material, with the change of office.

The President: No, the material is with the treasurer, not with the secretary, according to the Constitution.

(It was explained that the plates had, by special arrangement, been left with the secretary.)

Professor Ward: Plates to be useful at all must be used in a very short time. The society will not print the same plate in a volume within a year or two. After that it is of little use. But

this does not affect the question of considering a permanent home—for the two questions must be distinguished clearly.

The Secretary: One paper in the journal this past year was illustrated with a plate used in 1884, and last year Dr. Stedman applied to me for a plate used in 1882.

Dr. Mercer: Such plates would be very useful if in the hands of the society for illustrating historical papers treating of the development of some subject—as I have seen in articles published by the Royal Microscopical Society.

The President: The question is on the amendment, that a committee of three be appointed to investigate the whole matter of the property of the society and a permanent home for the society.

The President put the motion, which was carried.

The President: A proposition has been made to print the Constitution and By-laws—four pages—every year in the proceedings, so that all new members may receive it and know their rights and duties.

The Secretary: The proceedings are arranged to bind two years, in a single cover. Every two years there is an index for this double volume, there being only tables of contents with the annual volumes. The Constitution has been put in only once in this biennial volume, not in each annual volume.

After discussion, Professor Rowlee moved that the Constitution be inserted in each annual volume.

Mr. Pflaum: I move as a substitute that the Constitution be printed separately for distribution.

The President: And not bound up in the bound volume?

Mr. Pflaum: No, because not necessary. We could send these pamphlets to persons wishing to know the character of the society.

President Gage: The American Association for the Advancement of Science prints its constitution in every volume of its proceedings; the constitution and list of members are also printed separately for distribution, as suggested.

Mr. Pflaum: I then would make an amendment that they be

printed in the proceedings, and also separately for distribution. I would include the list of members.

Professor Rowlee accepted the amendment and the motion was carried.

The President: Our treasurer has had bound an official copy of our proceedings. It is moved that an official copy be similarly bound for the secretary.

Dr. Mercer: It would be well that this set, instead of looking exactly like the other, should be marked as being the secretary's official copy.

This amendment was accepted by the mover and the motion as amended was passed.

The President: The Spencer-Tolles fund is approximately $400, and is invested in a building association or fund in Ohio, netting six per cent. The treasurer proposes to transfer it to a similar association in Pittsburg where it will net ten per cent., and be safe. The executive committee has referred this to the society for action.

Mr. Pflaum: This building and loan society in Pittsburg is not a merely speculative and insecure one. It is national, and its officers, whom I know personally, are well-known and perfectly trustworthy men. In seven years the fund will be at least $1,000. The association has been paying fifteen per cent., but interest is falling everywhere and they do not expect over ten per cent, soon

The Secretary: This is an important matter and not of the kind that we as a society are well fitted to decide. It depends on judgment and investigation. I make a motion to refer the matter to a committee of gentlemen residing in Pittsburg, three members to be appointed by the President, with power to act if in their judgment it is the best thing to do.

Mr. Pflaum: I amend the motion by saying that the full membership of this society residing at Pittsburg be the committee.

The Secretary: I accept the amendment.

Mr. Milnor: I approve of what Professor Seaman has said. The society itself should not attempt to judge at a distance.

We should investigate thoroughly before the money is placed in the hands of any society, especially a building and loan society.

Dr. Mercer: I utterly oppose the investment of the money in that way. Mr. Spencer and Mr. Tolles were men who would have little sympathy with such a proposition. They would prefer three per cent. in a safe place, to twenty per cent. in a building society. If Mr. Spencer understood the principles on which these societies are conducted he would oppose it. As I understand it, they advertise to investors a high return—ten or twelve per cent., and to borrowers remarkably cheap loans. But that is impossible. It is the old story—the man who buys his coal by the bushel pays a higher price. Mr. Spencer would be the last to make him do that.

The Secretary: The object of the motion is that the committee shall do what in their judgment seems best—and I think the society will be willing to trust these members. I think Dr. Mercer must be under misapprehension as regards building associations, for statistics of the United States show that no other investment is so safe. But there are different kinds of associations. I should want to know the character of the men.

Mr. Milnor: I think if the matter is left to gentlemen in Pittsburg it will be safe. I know this association to be first class. But it has brought its rate of interest down now and will eventually be much lower, so it may not bring over six per cent.; and if that is the case I think bonds at six per cent. would be a preferable investment.

Dr. Mercer: I do not think we ought to place this money in any institution where any person can even use the word suspicious with reference to it.

This fund has been obtained with great difficulty and must not be lost. I believe it should be taken from Ohio, and it would be safer to put it into a savings bank at Pittsburg where it would probably pay four per cent. Building associations are not the place for trust funds. I should be afraid of an institution paying ten per cent. when money is begging everywhere at two per cent.

Mr. Pflaum: If the money had not already been in a build-

ing association I perhaps might not have suggested putting it in this one. This one is better than that. A building society is only a co-operative savings bank. The reason it pays such large profits could be readily explained; it depends on the compounding of interest largely. The members get the difference between six per cent. and ten per cent., taken by the proprietors of savings banks. These associations have been of greatest benefit to the poor men of Pennsylvania. The former opposition to them—*e. g.* that of the courts, has been overcome by their results. I think a committee could be trusted to treat this as a sacred fund, as it is.

Mr. Young: As I understand it the committee is not bound, by the motion, to change the fund or to put it in a building society, but as they see fit.

The Secretary: Yes, the resolution reads that the members of the society residing in Pittsburg shall form a committee to invest the Spencer-Tolles fund.

Mr. Pflaum: (Answering a question.) There are about seven active members in Pittsburg. It might be better that the President appoint three or five out of these, as it would take some time to sound all of them personally. I withdraw my amendment and leave the original motion for a committee of three or five.

Mr. Milnor: I want the responsibility to come from the Association—let the President appoint the committee.

The President: The motion then is that a committee of three be appointed by the chair to re-invest the Spencer-Tolles fund.

Mr. Pflaum: The change is necessary. The only member at the time of the investment in Urbana, Ohio, has resigned, and there is not a soul to look after the money there.

The motion as stated by the chair was now put, and carried.

The Treasurer presented his report.

Dr. Mercer: I move that the report be accepted and adopted, and that we thank the Treasurer for bringing to our attention the character of the investment of this fund.

The President put the motion, which was carried, and then

stated that the executive committee recommend that 500 copies of the proceedings be printed.

Mr. Pflaum: This matter might perhaps best be deferred until a permanent place of storage is decided on. As it is, our extra copies take about 200 cubic yards and it costs $12 a year to store them, without insurance. We have about 225 active members.

Dr. Moody: I move the society publish 300 copies.

The Treasurer stated the number of copies of each issue actually on hand, viz.:

YEAR.	NO. COPIES.	YEAR.	NO. COPIES.
1878–79	74	1890	57
1880	30	1891	246
1881	34	1892—Part 1	185
1882	43	Part 2	224
1883	26	Part 3	276
*1884	6	1893—Part 1	53
1885	44	Part 2	53
1886	35	Part 3	126
1887	236	Part 4	152
1888	154	1894	
1889	175		

*Of these numbers the Secretary has several copies. If any members could supply duplicates of 1884 to the Treasurer it would add to our complete sets.

Professor Ward: It is evident that even of the number in 1893, of which we have the most on hand, 350 copies were used. We can never tell when one of the papers may find a considerable sale—they often do. If we get a permanent home it will be no trouble to store even 250, while the lack of numbers at any time when they are really desired is a very serious matter. The cost of printing extra copies, not requiring type setting, is comparatively very little.

The President: The question is on the amendment changing the 500 to 300.

The amendment is lost.

Now we will proceed with the original motion, that 500 copies of the proceedings this year be published, and also as many copies of the Constitution, By-laws and list of members, as in the wisdom of the executive committee is desirable.

The Secretary: The society furnishes to each author in the proceedings 25 "separates" made by dividing up 25 of the 500

copies printed. As the Constitution, By-laws and list of members are to be printed in each volume, this separation leaves 25 copies of them, which can be used as desired. I have always furnished lists of members, to people asking for them, out of these copies. There has never been demand for more than that.

Thereupon the motion as above stated by the chairman was carried.

The President: The next question is whether the proceedings shall be printed as a single volume, as was done until the Rochester meeting, or in four separate numbers, as has been done since. The proposition is submitted by the executive committee without recommendation.

Professor Rowlee: I move the printing of the proceedings in a single volume.

The secretary just elected stated to me, knowing that the matter was coming up, that he thought it highly advisable that the proceedings be in a single volume. He did not believe he would be able to bring out four separate numbers and do it promptly. The work has been very hard for the present secretary. It really requires four times the technical work and care. It keeps one struggling the whole year instead of three months. I believe the incoming secretary can keep his promise to get out the entire proceedings of this meeting before the holidays. The present method was an experiment. Dr. Seaman was willing to give us the necessary time, as he has done; but he could not keep it up permanently. In spite of Dr. Seaman's best efforts the numbers have at times been delayed, and the work has been severe.

The President: I would prefer one number, and to look that through, and not to have to tie the four up with a string before they are bound. I have had a good deal to do with the printing —of course only a fraction of what the secretary has done, and I am convinced that to ask anyone to go over the task four times is asking too much. Editing the whole proceedings need not be much more work than one part. A person capable of being secretary can hardly afford the time. The last number for last

year did not appear till after the meeting, and this year's last number will not come out until after this meeting, in spite of our best efforts.

The question is that the proceedings for this year be published in a single volume.

The motion is carried.

Professor Ward: There is to be started with the beginning of the year a scientific undertaking in Europe that is to be international and of such a character that I want to bring it prominently before you, and urge action of the society in three directions. Most of you know the great difficulty of tracing bibliographical references, the extreme incompleteness of the present system of Bibliography. There is to be established in Zürich a bibliographical bureau for zoology, which will go into operation January 1, 1896. It absorbs the *Zoologischer Anzeiger*, and the *Jahresbericht*, which is published at the Naples station; it absorbs the *Archiv für Naturgeschichte*, at least the second volume, which publishes a résumé of the work in the different portions of the animal kingdom for each year; it absorbs some other minor bibliographical publications; and it is hoped another year that it will include also the English *Zoological Record*, thus giving in the scope of a single enterprise all the bibliographical notices of the world. In France a subcommittee to push the matter is very thoroughly organized, and the organization of the American side is thoroughly under way In France the subcommittee is under the support and encouragement of the Zoological Society of Paris. Sub-committees have already been organized in Switzerland, Italy, Hungary, Russia, Germany and England. The members who furnish the bibliographical records for the bureau give their services free. There are two or three paid officials in connection with the bureau itself, one of whom being the very well-known Professor Carus, of Leipsic, editor of the *Zoologischer Anzeiger*, who is to give his entire time for the munificent sum of $500 a year. This shows you that the enterprise is a purely scientific one. The men who are working in it are not doing it for the sake of

gain, but in the effort to lighten the labors of the workers in this field throughout the world.

The bureau proposes to record all publications which touch upon zoology, and to have the recording done not by librarians—and you know that the cards sent out by library associations for scientific topics sometimes include Rotifera among the Infusoria, and make all sorts of mistakes—but by zoologists who know exactly what they are doing. The bureau is to have two publications, a fortnightly bulletin and a card catalogue. The bulletin is to be in exactly the same shape as the lists and short reviews or résumés of contents now given by the "*Zoologischer Anzeiger*." The card catalogue is to have these same, but each printed on a card after the fashion recommended by the Library Association.

It is expected to receive subscriptions to both of these, and also that the bureau will be able of offer specialists subscriptions to that portion of the bulletins or cards which deals with their special work—as for instance that part of the bulletin dealing with entomology and the cards dealing with that subject.

There are three things that the bureau asks, and must have if it is to succeed. It asks in the first place a certain subsidy, for in the first two or three years it is practically certain that it cannot pay expenses. The Swiss, French and German governments have already granted subsidies—the Swiss gave $400. I had an interview in New York with the gentleman who is at the head of the whole enterprise, who has put all of his time, and of his fortune—not a great one—into the undertaking. He told me the particulars of the votes by the several governments, but I have forgotten some of them. The government in this country can not take such a part as this. The matter can only be pushed by bringing it before the learned societies. In this country a subscription of $250 in addition to what has already been raised is all that is asked. This is not an exorbitant share, I am sure. The German Academy of Sciences, let me say, has voted a subsidy to the undertaking. There are three societies here that can be looked to for a subsidy—the Association for the

Advancement of Science, the American Society of Naturalists, and our own society. What else is collected must be from private persons or local societies. I know it is difficult to raise even a small sum. The executive committee has discussed it, and it is easy to see that the society is not rich. But I should like to ask the society for $25—a very small sum. I ask this more for the reason that I want to see the society in line with this work, which is for the benefit of the whole world and of every worker in this country.

Second, the bureau asks that we send a free copy of our publications. And third it asks subscriptions for its publications, but of course this concerns private persons rather than this society.

The President: If the chair may be allowed, I should like most heartily to second this motion. I may add that since the meeting of the executive committee, I have received a letter from Dr. Kingsley saying that the combined societies in the American Society of Naturalists have given a very cordial endorsement of the movement, and that he counts on us for help. "Cannot and will not you help us?" is the last sentence. It seems to me that our society which publishes proceedings, wishes them to be made known widely in just this way—we do not want to be in a corner. I am happy to say that on talking with some members of the society I can say that if this vote is passed by the society it shall not come out of the treasury or from the ordinary income of the society. It shall have the honor, if it is such, of making the gift, but it shall not bear the expense.

All those in favor of giving our proceedings to this Bibliographical Bureau, and of granting a subsidy of $25, please say aye.

The motion is carried.

(It was stated by Professor Ward that this subsidy was for one year only and did not bind the society after that.)

The President: I think that finishes the business of the society, except for what we recognize as a pleasant function as well as one of our duties, and that is our words of appreciation for the kindness of the people of the place at which we meet.

The first vote, I think, we all recognize, should be given to the University for the welcome that has been given us, and for the facilities offered for this meeting.

Professor Ward: We must all recognize that the success of the meeting has been largely due to the place in which it has been held. Entirely aside from the fact that Cornell University has honored us with "A. M. S.," witness these buttons—and a Master's degree from Cornell University is a matter of congratulation certainly for each of us—entirely aside from that, the cordial reception which has been given us by the officers of the University, the way in which everyone has made us feel at home within these walls, will cause all to carry away a feeling that Cornell University keeps a unique place in our thought. To those of us who are college men no affection can be quite like that which one feels for his *alma mater*, but I must confess that after the very pleasant time which I have had here, there is a second place, comparatively near that, which will be given to Cornell University; and it is with a great deal pleasure that I move that we extend to the President, the officers and faculty of Cornell University our sincere thanks for the very cordial invitation and reception which they have given us, and for the facilities which they have placed in our hands, aiding us in holding the most successful meeting of the society which it has been my privilege to attend.

The President: You have heard the motion, ladies and gentlemen. All those in favor of this motion will rise—that is the kind of voting one likes to see.

Next to the University, I think, we have to thank our local committee. If anyone has ever been a member I feel quite sure he would agree with me in that.

Mr. Seaman: Mr. President, having been secretary of the Society several years, I think I am in a position to say something about the local committee. The first year in which my active duties as secretary began was at Washington, and there we had an active local committee. The next to the last year of my duties was at Brooklyn, where there was no local committee and

nothing else. Now the difference between those two extremes of conditions measures the difference between a successful meeting of the society and a failure. There is nothing that can make up for a want of interest in the place where the society meets, as regards some at least of the conditions of a successful meeting. Of course, if the society is strong in members, and those members attend from all over the country, they can have a good meeting where there is nothing but a house to cover them. But that is not usually the case. There are certain requirements of attendance, of providing for the comfort of members, of showing interest in the meeting which contributes enthusiasm to those who come from a distance; and more than that, if we look over the meetings of the Microscopical Society, you will see that on several occasions in the early days of the society the meetings have been redeemed from failure by the activity of one or two men in the place where the meeting was held. I think that I might refer to the meeting at Elmira particularly as an instance. So the meeting at Columbus, Ohio, which was one of the first that I attended, was such that it seemed to me at that time that the society had touched a very low ebb, and had it not been for the efforts of one or two of the local members there would scarcely have been any meeting. Now in a society that is in the early days of what we hope will be a prosperous future, the local committee is one of the principal pillars upon which it must rest. And in the case of our meeting here, I can say most heartily and earnestly that the local committee has done the largest part of the work, that to it is largely due the success of this meeting. I therefore offer a resolution of thanks to the local committee from the very bottom of my heart, that its members have lightened my labors and made the meeting a success.

The President: I think that I will proceed with the original method and not take a vote, because I am sure that is in the heart of every one of us.

Next to the local committee we have to thank the city of Ithaca. As I said in my remarks in answer to the address of welcome, we had not hoped so very much from the city. That

was wrong in us. We misjudged the city. When we went to them we found their hearts open, and they stayed open; and the next time we have a meeting we shall not go with any doubt to the men of the city, but with full confidence. I hope there will be given in the heart of every one of us a hearty vote of thanks for the city, and I would call upon Dr. Moody, who was a student here for many years, and who knows the city better than I do, to say a few words to us concerning it.

Dr. Moody: I must confess that I felt a little surprise when our President announced that he approached the people of the city with what we might call fear and trembling, with regard to the reception of the society; because it was my experience as a student here that when anything was started on the campus the town was always behind us. Surely in this case it was so. I heard the chairman of the local committee the other day on the street telling what great success he had had in the work he was doing among the citizens. I am sure we all felt from the time that we were welcomed by Mr. Van Vleet at the opening of the meeting, that the hearty welcome he gave us, came from his heart and from the hearts of the people—we felt that their hearts were open to us. And I am sure as we sailed down this beautiful lake yesterday afternoon, amidst your delightful breezes, we all felt then that the citizens had an interest in us, and that the street-car company also, which carried us up and down these hills, had an interest in us. I feel that each one of us will be glad to come to Ithaca again, where we have received so warm a welcome, to accept the invitation which has been extended to us indefinitely for some future time. I therefore move a vote of thanks to the citizens of Ithaca for their cordiality to us.

The President: I would like to put the motion that has just been made, namely: that we render the citizens of Ithaca a hearty vote of thanks for their courtesy to us, and their warm welcome to the city.

The motion is carried.

The President: When the chairman of the local committee and myself were discussing this meeting, the question of the press

arose immediately in our minds. What help could we get in giving information about the society, and for the encouraging and interesting of the people here in Ithaca in the society? The chairman of the local committee went to the papers in Ithaca, and the welcome there was cordial. You who have seen the papers since you have been here I know cannot help feeling that the press of Ithaca has done everything that it possibly could to make our meeting a success. They have acted, as the Honorable Mr. Van Vleet said in the beginning, as though they felt honored in the presence of the society in their midst, and they have done for us, if one remembers the limited space of the great papers nowadays, a great deal. I would ask some one to say a few words on what the press has done for us in our meeting.

Mr. Pflaum: A sure test of the grade of civilization of a race, of a nation, lies in the short phrase, "What's going on?" —both in the question and in the answer, and especially how that answer is recorded, so that neither wind nor weather, nor time, even, can ever blot it out. The question with the savage refers to immediate bodily needs, and the answer dies with the acquisition of his physical requirements. With advancing civilization the question broadens in scope, and takes in friend and neighbor, and gradually it leaps over territorial bounds till finally it takes in all of society, and records its answer for future generations. How painful is the attempt of our remote ancestors to tell us what was going on. The invention of form writing, of hieroglyphics, to tell even a simple event, must have taken years of the writer. With the invention of the alphabet the recording of that answer was made more easy, but the process remained at best a rude and slow one. It was the invention of printing that gave the greatest aid. The art of printing gave an impulse for more questioning and a wider answer. To-day the question and answer to "What is going on?" has become centralized in the most civilized nations, and their standing is measured by the quantity, quality and freedom of that grand modern organism known as the press.

Our society most gratefully acknowledges that our work and

aims have been recorded and spread broadcast as part of the answer by the press to the question, "What is going on?" We have been aided and benefited, and our labors lightened by the help that is has afforded. The press of this city has nobly performed its functions, it has faithfully recorded our endeavors to add our part to human advancement. We appreciate the kindly feeling and generous spirit of the press of Ithaca, and tender it a most cordial *au revoir*.

The motion of thanks to the press of Ithaca was put and carried.

Professor Rowlee: I should like to make a few remarks in regard to the privileges that have been extended to us in another relation, and I think I can do so without blushing. I think many of the members do not understand how much we have to thank the railroads and the street railroads of this city for the success of this meeting. It was with a good deal of embarrassment that I asked for the privilege of reduced rates to this meeting. I feared that there might be some question as to whether we were entitled to the privilege. I expected when I went to meet the agent of the Trunk Line Association to find a strictly business and railroad man, who would be very strict in his treatment of us. But when I met the gentleman his first words were: "We want to do for you people everything that we can. We realize that you are working for the advancement of civilization in this country, that you are doing it freely, and we want to do everything for you that we can." He has treated us in every way most cordially. I am sure that any man who has to do with this gentleman, or with the other railroad men of the country, in regard to meetings for the furthering of human knowledge, will have a very cordial reception, and he should not be afraid to ask for anything.

In regard to the street railroad, the mere asking was the having of the privilege of your riding free over their lines to the lake. I did not ask them to carry you free all the time you were here I am not sure but they would if I had asked them. The President of this society has often reminded me that when

he wanted anything done that required a great deal of courage —he did not call it courage, however, he called it cheek, but I think courage is a better word—he sent for me. But my courage failed when I thought of asking free transportation during the whole meeting.

I move that we tender a vote of thanks to the Trunk Line Association of the Central States, and to the Street Railway Company of Ithaca for the courtesies which they have extended to us during our meeting.

The motion is carried.

The President: This completes our program, I believe, with one exception, and that is the pleasant duty of the President to resign his chair to the incoming President. I wish to say that I have only sincere gratitude to the society for the cordiality that I have received at their hands. I hope that the incoming President will receive the same. I feel sure that he will, and I think that if we go on with the society, each one of us trying to do the very best he can, going in the spirit of true scientific brotherhood, there won't be any question ever arising as to whether we are being treated properly or not. We won't think anything about it. It is like the best kind of digestion; we don't know that we have any stomach while we are all right. It is only when we are dyspeptic that we know it—that we feel as the little boy did after the Thanksgiving dinner, when he asked his mother if the turkey was alive again. There will never be any question of that kind with us. We are helping our friends; they are helping us. I am frank to say that a great many of the things that have been stated on this platform have given me the greatest help, and real inspiration. It perhaps may not be modest for me to say it, but it seemed to me as I listened to these papers, that in a great many of the societies I have attended before, we have not had any that have exceeded these, to say the least. If I may say in a general way, not to be egotistic, it has done my heart good to see the genial look on the faces of the people as I looked into them. Nobody seemed to feel that he was being abused; nobody looked as if he was not enjoying

himself. Everybody seemed to think that things were going rightly, and that look encourages the presiding officer and the one who is reading the paper, and thus it makes the meeting a success. Dr. Mercer—when acting as President, the best I can say for you, the best wish I can have for you, is that you will be accorded the same pleasant treatment.

Mr. Milnor: I think that one thing which always makes these gatherings pleasant—a great deal, at least depends upon it—is the manner in which the presiding officer discharges his duties. And I think we all would like to express our appreciation to the President of the past year for the manner in which he has discharged his duty, his courtesy and the way in which he has made us all feel perfectly at home. I regret exceedingly that this task of expressing to him our feelings, the most pleasant one, I think, of all, has not been placed in the hands of some one more eloquent than myself, one that could speak forth the sentiments that I know have filled the hearts of every one who has attended these meetings. I therefore with the utmost pleasure—it is one of the greatest pleasures of my life—move that a standing vote of thanks be extended to Professor Gage for the manner in which he has discharged his duty to this meeting.

(Applause and a standing vote of all the members.)

President Gage: I thank you gentlemen, most heartily, for this vote of thanks.

Inaugural remarks of the new President.

President Mercer: *Fellow members*—I thank you for the honor that you have conferred upon me by your choice. It is an honor which I feel, and an honor at the same time which I know perfectly well means something in the way of work. So far as I am able to work for you I shall be happy to do so. I hope that your wishes will be freely expressed in one and another, so that I may know what to do.

If I could carry that face, which is beaming and smiling before us, as our old President did, I have no doubt I should see beams and smiles in the faces all around me. He forgot in wishing for

me the same cordial reception, that he but saw round about him the reflection of his own face. I think it is a rather hard thing for a successor of Professor Gage to get those smiles. However, I am at your service and your pleasure.

The society then adjourned.

Friday evening, August 23, at eight o'clock, the society held a soirée in the University gymnasium and armory, which was attended by a large number of visitors from Ithaca. About sixty microscopes were in use, and those present expressed a great deal of pleasure at the opportunity afforded to examine objects by the microscope. As one of the incidents of the occasion, Professor Burr, who entertained the society so pleasantly with his literary treasures, assured the writer that, although connected with the university several years, he never before had looked through a microscope. This entertainment closed one of the most successful meetings the American Microscopical Society has ever enjoyed.

[The letter of Dr. Seaman resigning the position of editor and secretary of the society, also the resolutions expressing the society's thanks and hearty approval of his labors, have been misplaced aud could not be obtained by the present secretary to insert in the proceedings.—*W. C. K.*]

TREASURER'S REPORT.

FOR YEAR ENDING AUG. 21, 1895.

RECEIPTS.

Cash on hand at Brooklyn Meeting		$180.32
Membership Dues for 1888— 1	$ 2.00	
" " 1889— 1	2.00	
" " 1892— 4	8.00	
" " 1893— 4	8.00	
" " 1894— 25½	51.00	
" " 1895—202⅗	405.20	
" " 1896— 6⅖	12.80	
		489.00
Admission Fees for 1892— 1	3.00	
" " 1895— 10	30.00	
		33.00
Sale of Proceedings		12.80
		$715.12
Brooklyn Prize Fund		30.00
		$745.12

EXPENDITURES.

Postage	$ 29.41		
Expressage	13.82		
Stationery and Printing 1894	1.75		
" " " 1895	16.75		
Sundries	3.75		
Binding Official Copy of Proceedings	10.50		
Issuing Part 4, Vol. XV	177.73		
" " 1, 2 and 3, Vol. XVI	394.49		
		$648.20	
Cash on hand at opening of Meeting at Ithaca		96.92	
			$745.12

SPENCER-TOLLES FUND.

Reported at Brooklyn Meeting	$349.38
Interest received during year 1894–95	22.89
	$372.27

MAGNUS PFLAUM,
Treasurer.

We hereby certify that we have examined the foregoing accounts, and find the same correct with proper vouchers for expenditures.

F. W. KUEHNE.
D. S. KELLICOTT.

Ithaca, N. Y., August 22, 1895.